

Interactive comment on “Homogeneity criteria within IASI pixels for the preparation of an all-sky assimilation” by Imane Farouk et al.

Anonymous Referee #2

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General comments:

The English is generally very good, the paper is well structured and nicely concise. I am proposing major revisions because some of the plots should be remade, and the scope of the paper does not seem to fit the ambitions of the title. I found the choice of experiments a little strange, and was rather confused on the details. However, whilst I am suggesting some extensive changes, I think that these matters could be fixed quite easily by the authors.

I realised when I reached the final sections of this paper that I had totally misunderstood its intention. I had expected that the homogeneity criteria would be used to select additional observations, that are homogeneously cloudy, to assimilate in addition to the clear-sky channels accepted by the McNally and Watts check - i.e. to do something

similar to McNally (2009). It took me almost the whole paper to realise that what is being proposed is additional quality control on radiances already accepted by McNally and Watts. This does not seem like “preparation for all-sky assimilation” and I think that the scope of the paper should be revised.

At least, I assume that it is the case that this is just extra QC on clear sky calculations. . . I confess that I found the paper surprisingly confusing! There is no mention that RT-TOVCLD is being used in the assimilation experiments, therefore I assume the calculations are clear sky, and there is no mention of the use even of a single grey-cloud layer scheme in use as in McNally (2009).

Some curious choices are made throughout the paper: I believe that the E2014 method was adapted because Eresmaa’s intention was to keep only clear scenes, whereas you wish to allow through homogeneously cloudy scenes also, but in fact because you are rejecting observations that had already been allowed through by McNally and Watts, I don’t see why you don’t just apply Eresmaa’s method without modification. Why do you not include the scheme of McNally (2009)? And finally, perhaps most surprisingly, you use the AVHRR clear/cloudy pixel fraction as a measure of whether the homogeneity criteria have “correctly” picked out homogeneous scenes, and on p15 you state that you are happy to accept a reasonable proportion of observations with >90% cloud cover. Why not just test the use of the AVHRR clear pixel fraction? And yet, you performed assimilation experiments with a scheme that you had seemed to reject based on the O-B statistics presented in Figure 4.

It is not really surprising that there is little impact, as very little seems to have changed in the experiments relative to the control. The work therefore seems rather immature for a publication. You still apply the CO₂-slicing method (p16 line 17) - presumably this is designed to reject cloudy scenes? What effect does this have on the homogeneous scenes?

Specifics: P9 line 7: it is not clear whether this 7K check is an additional criterion

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over the 8% criterion in the previous paragraph. Why 7K? I also don't understand "interpolated using 12 points" - is that 12 points in 3D?

P10 line 14-17: I don't understand how this first bullet point is different from the original method P10 line 18-19: Why 49K2? Other than that it fits the 7K applied to M2013, it seems quite high relative to 1K.

P11 line 5: This is the same as the first test of Martinet but with 2 channels. It would be clearer if this was stated. Why change the L to R in the equation? What does the addition of the second channel bring in practical terms?

P12: What is this dataset of 59 million observations? Is it just 24 hours' worth of observations? You state that 50% of the observations are 100% cloudy - that sounds potentially high for a normal dataset?

P12/Figure 3: Are the numbers in the text for bias and SD an average over a number of channels, or the maximum value from the windows? It should probably be the latter. I cannot match the figures in the text with the plots - the numbers do not seem to match (e.g. 11.7K bias -> the bias looks over 12 K in the figure). It would also be better to just plot Band 1 so we can see the effect on the temperature channels. I think the numbers scattered over several paragraphs and two pages would be better in a table.

P14 line 15-20: I would disagree that the distribution asymmetry is small. I also disagree that the Obs_HOM approach reduces the range of the tropospheric water vapour channel distribution.

P15: The discussion focuses on letting through the most data - this isn't necessarily the best criterion, as you may be letting through inhomogeneous scenes. There is trade off between more data and better data. M2013 and Obs_HOM let through a lot of partially cloudy scenes (and even 100% cloudy scenes may have different cloud types in one pixel).

P16: It is not clear what the set-up for the assimilation experiment is - you do not

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mention RTTOVCLD - presumably this is still clear sky.

Figures: Figure 1: would be better as two bigger O-B plots. Figure 2: very strange Y-axis. You can't see much on these plots. Is the Y-axis expressed as % as in the criterion on p11 line 11? Figure 4: Why not plot a temperature sounding channel? The x-axis has strange divisions. It would be better symmetrical. Figure 6: I honestly cannot see any difference between these three plots. You need to revise the colour scale to highlight the differences. Figure 7: I cannot see the REF line: is it under the green line or the red line? This is an important figure as it is the first time I realised this paper was about improved QC (more obs are assimilated with Experiment B than A).

Tables: Table 2: Should include the % partially cloudy Tables A1 and A2 are unnecessary - this information is presumably included elsewhere. If not, a simple list of channel numbers would suffice.

Minor points:

P2 line 9: seems to be the first use of IR without the abbreviation being expanded. Section 2: this section is a light-touch description of the model and IASI, as it should be, but it is important to get the details correct in that case and make sure the writing is clear: P4 has a few poorly worded sentences, or poorly explained concepts. P4 Line 7 - the background error statistics are not "derived from a climatological matrix" - it isn't actually a matrix, and you do not explain how the ensemble information is incorporated. P4 Line 22 - this area needs rewriting - Presumably you mean that the accuracy of the forward model calculation is limited by the accuracy of the NWP model, and that for some variables this is not sufficient to correctly model the observations? "Modelisation" -> "Modelling" in English! P4 Line 29 - The McNally & Watts scheme is not clearly described. P4 Line 31: In this section, there are numerous references to CTOP and Ne, but suddenly you switch to PTOP - maybe Pangaud (2009) used PTOP instead of CTOP but this switch is not necessary.

P5 line 1: "IASI is a key element of the payload of the Metop series of European. . ." P5

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para 1: you may as well update this with Metop-C launch date P5 line 22: It's a bit far to say that failing to assimilate cloudy IR observations is a source of error. P5 line 25: "allows to better describe" - not good english "allows a better description of. . ." P5 line 29: You should reference the Baran parameterisation if you are going to mention it.

P6 line 8: "an innovative challenge"? Remove the word innovative. P6 line 8: The sentence "In the context of. . ." doesn't make sense. P6 line 21: They are not IASI L1c products - they are components of the L1c product. P6 line 24: this sentence is not clear either. P6: line 28: this sentence is not clear. I think it is a stretch to say something with one class can be less homogeneous than something with multiple classes - this is a bit subjective.

P9 line 11: "aimed to propose" - that is a bit of a negative slant on this reference! "Proposed" would be better!

P17 line 15: No need to reference Table 3 here - it is a very basic table and you describe it all in the text,

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